

Translational Contributions of the Experimental Analysis of Behavior

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It has been argued that to increase societal impact behavioral researchers must do more to address problems of obvious practical importance. The basic science wing of behavior analysis has been described as especially detached from this goal, but is it really necessary that basic science demonstrate social relevance? If so, why hasn't this occurred more often, and what can be done to improve the status quo? To address these questions and to stimulate discussion about the future of basic behavior science, I describe two widely embraced arguments in favor of pure basic science (that which is undertaken without concern for practical applications); explain why a translational research agenda is likely to better recruit tangible support for basic science; propose that addressing practical problems does not require basic science to abandon its focus on fundamental principles; and identify some possible impediments to translational innovation that may need to be addressed for basic behavior science to increase its translational footprint.

Key words: experimental analysis of behavior, translational research, public policy, dissemination

Intellectual movements either evolve or stagnate, so one hopes that tomorrow's behavior analysis will not look exactly like today's. Toward this end, the contributors to a recent special section of *The Behavior Analyst* (Schlinger, 2010) discussed how our field might productively innovate by connecting with mainstream science and culture. The authors agreed that behavior analysis can improve both society and its status within society by tackling problems about which laypersons and diverse scientific communities care deeply. They also agreed, however, that considerable work lies ahead before this goal can be accomplished. The bleakest assessment in this regard was offered by Poling (2010) in his evaluation of the field's basic science wing (normally labeled after its methodological canon, the experimental analysis of behavior, or EAB). Poling described EAB as a weak force in the collective drive for greater social impact, going so far as to suggest that the acronym EAB implies an "esoteric" (p. 8) analysis of behavior. "Many basic

research studies," Poling asserted, "are not obviously relevant to significant actions of [individuals] in their natural environments" (p. 9).

Poling's (2010) assessment of EAB, although pointed, is not entirely novel. For instance, Roediger (2004), in discussing why behavior analysis holds a less prominent role in psychology than it once did, suggested that some observers may have concluded that,

Behavioristic analyses were becoming too microscopic. ... As in most fields as they develop, researchers began studying more and more about less and less. Rather than focus on the central, critical problems, behavioristic researchers begin looking at ever more refined (that is to say, picayune) problems, with experimental analyses increasing in complexity all out of proportion to the gains in knowledge that they enabled. (unpaginated electronic source)

Readers who have struggled to make sense of a densely technical, heavily theoretical article in the *Journal of the Experimental Analysis of Behavior* (JEAB), and wondered about its relation to events taking place outside the specialized laboratory environment, may find sympathy for Roediger's suggestion.

Poling's (2010) call for basic research studies that bear directly on

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practical problems is consistent with a contemporary trend toward *translational* science (e.g., Mace & Critchfield, 2010). Translation often is conceived as exploring the insights that applied researchers and practitioners can derive from basic science, but Poling echoed a point made by several previous writers: Basic researchers bear responsibility for promoting translation by selecting research problems that directly target socially important problems (e.g., Cullen, 1981; Hake, 1982; Mace, 1994). In surveying the translational history of behavior analysis, however, Mace and Critchfield (2010) concluded that, although some noteworthy contributions have originated in the EAB laboratory, basic researchers have indeed contributed less than their applied counterparts toward a translational agenda. Given the importance that is placed on translation in contemporary scientific circles (e.g., Perone, 2002), Poling's critique may prompt the reader to wonder: What is wrong with the experimental analysis of behavior?

I take it as self-evident that more could be done in the EAB laboratory to demonstrate social relevance. Whether this really should be the case and, if so, how change might be effected, are matters on which intelligent people may disagree. The present essay is intended to promote constructive discussion on these matters. Regarding whether EAB should adapt to increase its translational footprint, I focus on the problem of justifying EAB as worthy of investment by a society with practical values and limited resources. Regarding whether EAB can adapt, perhaps the only certainty is that one cannot simply will EAB into greater social relevance. If we are dissatisfied with the status quo, we must examine basic behavior science for what it is, behavior, that is, what members of the EAB community do (e.g., Skinner, 1957). As with all behavior, the starting point for change is an

analysis of controlling variables. Toward this end, I identify some historical forces that may have discouraged a robust translational perspective in EAB. I also explain why, for present purposes, the alternative view that basic science is independent of practical concerns is logically flawed. Finally, I note that increasing the translational emphasis in EAB will require more than (as Poling, 2010, might have inadvertently implied) simply selecting new kinds of research questions. It may be necessary as well to consider such fundamental issues as how basic researchers are trained and what methodological tools are employed in basic behavioral science.

WHY TRANSLATION ISN'T MORE COMMON IN EAB

Although it emerges occasionally from the basic science laboratory (e.g., Mazur, 2010), translational research has been atypical in EAB, which appears to focus more often on theory and fundamental principles than on socially important problems. Why might this be the case?

At least since Aristotle it has been popular to assume that society is directly enriched by the quest for knowledge. From this perspective, no need exists for pure basic scholarship to justify its existence in practical terms. Knowledge is the only outcome that a scholarly initiative *must* deliver, and pure basic science therefore meets its obligation to society simply by advancing understanding (of course, some branches of science also will seek practical benefits, but this is, in a sense, orthogonal to the proposed central purpose of scholarship). Let us call this the *inherent benefits* defense of basic science.

If we trace pure basic behavioral science to B. F. Skinner, it is easy to see the inherent benefits assumption manifest from the earliest days of EAB. Neither of Skinner's major basic science works, *The Behavior of*

Organisms (1938) or *Schedules of Reinforcement* (Ferster & Skinner, 1957), attended to everyday issues, something about which Skinner was unapologetic: "It is a serious ... mistake to allow questions of ultimate application to influence the development of a systematic science at an early stage" (Skinner, 1938, p. 441). Skinner's "pure science" instincts, however, may best be summed up in the inductive prime directive of "A Case History on Scientific Method" (1956): "When you run into something interesting, drop everything else and study it" (p. 223). It is important to note that in this maxim, the definition of *interesting* is left open to investigator interpretation, with no necessary ties to social relevance. In the ways just described, Skinner provided an early, and influential, model of pure basic behavior science that appears to have been embraced by many throughout EAB's history.

At least since the Industrial Revolution the inherent benefits view has been accompanied by an *indirect benefits* assumption that claims practical high ground for pure basic science (Stokes, 1997). According to this view, although pure basic research does not directly address practical problems, it reveals regularities about the world that also are expressed in practical situations. Someone else, someday, can be expected to harness the resulting principles for practical benefit. From this perspective, pure basic research pulls its societal weight, even though individual investigators do not ask translational questions and the practical value of today's investigations is not immediately apparent.

The formative years of EAB coincided with an interval in history during which, it has been argued, the indirect benefits assumption was embraced with unusual zeal. Stokes (1997) has detailed how the indirect benefits assumption became a guiding principle in science policy in industrialized nations, particularly the United

States, after World War II. Among the consequences: Extramural funding was linked to pure basic science in unprecedented ways, and academic hiring, always sensitive to funding priorities, favored basic researchers as in no other period during the history of the modern university. In short, researchers who pursued pure basic research could, for a time, find jobs and scientific resources and could develop their investigations with thought only for charting the fundamental regularities of Nature.

According to Stokes (1997), the 1980s and 1990s were marked by a significant decline in influence of the indirect benefits assumption on policy-level support for basic science. A supportive environment had remained in place long enough, however, for a community of EAB researchers to develop across several generations of investigators who would have had reason to think of pure basic EAB as a sustainable venture and the preferred way of doing business.

I can provide no actuarial data on the attitudes of EAB investigators, but can attest that when I began conducting basic research in the late 1980s, considerable peer pressure existed to adhere to the pure basic ideal. For instance, I recall colleagues who, after being driven to applied positions by a collapsing academic job market for basic scientists, were derided as having "sold out" or "gone soft." There seemed to be limited interest among members of the EAB community in applied and translational issues. This disinterest was documented by Perone (1985) in the form of asymmetrical citations between basic research articles that can be considered more rather than less translational (with the latter regularly citing the former but not vice versa),¹ and

¹ More recent analyses show asymmetrical cross-citation between basic research in *JEAB* and applied research in the *Journal of Applied Behavior Analysis* (e.g., Elliott, Morgan, Fuqua, Erhardt, & Poling, 2005).

was identified by Hake (1982) as an impediment to translational progress.

There is another piece to the story that may help to explain why pure basic ideals have not been more energetically embraced in the basic behavioral science community. If EAB's infancy begins with Skinner (1938), then its adolescence can be equated with the early years of *JEAB*, which correspond to a time in which the American Psychological Association (APA) was rapidly losing status as a home for basic science (e.g., Laties, 2008). APA had instead become dominated by the concerns of private practitioners of clinical psychology, and basic scientists who lived through this era (and later scientists who were trained by them) may well have concluded that basic and applied concerns mix uncomfortably, with basic science bearing the brunt of the discomfort.

Overall, EAB's primary identity as pure basic science appears to have been shaped by powerful historical forces. We should not be surprised if EAB has spawned limited translational efforts. This is precisely how our field's basic science wing was designed to function, and it is difficult to criticize a branch of science for having, in effect, fulfilled its destiny. If anything, given this historical backdrop, we should be impressed by the volume of translational work that *has* originated in the basic laboratory (see Mace & Critchfield, 2010).

WHY A PURE BASIC PERSPECTIVE MAY NOT BE SUSTAINABLE

The concern of the present essay, like that of Poling (2010), is not whether EAB has occasionally spawned translational research, but rather whether, given its primary identity as pure basic science, EAB can survive in a world dominated by practical interests. Individuals may conduct basic science for many rea-

sons, including the joy of seeing orderly data that Skinner (1956) highlighted in his inductive approach. But regardless of why investigators choose to do basic science, someone must pay for it. Basic research requires resources and rarely is self-capitalizing (i.e., it does not tend to generate income in a free market). Thus, basic research can proceed only to the extent that its practitioners are independently wealthy (unlikely) or society is willing to subsidize the effort (possible, but not guaranteed). At issue, therefore, are the conditions that are likely to influence society's appreciation of basic science efforts. From the perspective of society, the operative question is not whether basic research yields benefits, but whether these benefits are perceived to justify the expenditures that are required to obtain them.

A Benefits-to-Costs Evaluation of Basic Science

Is pure basic behavioral research "worth it"? To approach an answer to this question, let us begin with Poling's (2010) contention that basic behavioral research, whatever its historical shortcomings, is at least inexpensive to society. Poling defended this claim by noting that the cost of a single nuclear submarine dwarfs that of all behavioral research conducted to date. Thus, in a ratio of EAB benefits to costs, the numerator is nonzero (according to inherent and indirect benefits assumptions) and the denominator is negligible, in which case EAB research seems to more than justify its own existence. If I disagree with Poling about anything, it is his definition of *expensive*. Submarines indeed cost more than, say, operant chambers or payments to human subjects, but no historical precedent suggests that society would divert substantial portions of a national defense budget into basic behavioral research. Rather, there exists a pool of resources that society

may consider investing in scholarship, and although the size of this pool cannot easily be quantified, we know that even under the best of circumstances it is finite.

Common sense also suggests that the demand for scholarly resources exceeds the supply. Thus, society's investment in any given line of research carries an opportunity cost, in that certain other lines of research cannot be simultaneously supported. Neither the inherent benefits nor the indirect benefits view addresses this point, but once opportunity cost is acknowledged, a more careful examination of benefits is required. Perhaps society *should* value abstract knowledge, but it faces many practical challenges and therefore has a right to consider the practical returns on its investment in science. From the perspective of practical returns, the inherent benefits argument for pure basic science is a bit of a non sequitur; it risks offering apples to a world demanding oranges.

Because of its focus on practical contributions of pure basic research, the indirect benefits argument holds up somewhat better. For example, Miller (1985) identified a number of effective psychological therapies that have their roots in basic behavioral research (much of which, it may be inferred, was not conducted originally with practical benefits in mind). That such cases can be identified is indeed important to basic behavior science: With the benefit of hindsight, the research programs to which those therapies owe their origins proved that they were "worth it." Unfortunately, this conclusion does not fully inform the present discussion, for the following two reasons. First, to judge the generality of cases like those cited by Miller, it might be important to know what proportion of basic science research programs can be expected to yield such practical benefits. I am aware of no relevant data in behavior analysis, but formal evaluations of other basic science domains

have not been encouraging. It appears that most pure basic research does *not* lead to practical benefits (e.g., Stokes, 1997). This could be true in some cases because, as Poling (2010) suggested, the focus of basic research really is orthogonal to practical problems, or in others because the analyses and language that serve basic science do little to fuel the imagination of those who might develop and disseminate practical innovations (Rogers, 2003). Second, it might also be important to know what proportion of practical innovations originate with basic research. Analyses in other disciplines suggest that only a minority do, with the remainder attributable to other sources like applied research and technological "tinkering" (e.g., Rogers, 2003; Stokes, 1997). Rutherford's (2009) account of the early days of applied behavior analysis echoes this theme to some degree. If basic science is seen as neither necessary nor sufficient to fuel practical advances, then it would not be surprising to see society direct its tangible support elsewhere.

Of course, even when basic research proves to be integral to practical innovation, there is danger of this connection being overlooked precisely because it is indirect. Miller's (1985) purpose in writing about indirect benefits, for example, was "to address the repeatedly-made assertion that behavioral research with animals is without any value" (p. 437). To illustrate the difficult discrimination that may be required to see the imprint of pure basic research on translational innovation, consider the example of research on discounting, which focuses on how the impact of consequences on choice is degraded by such factors as delay, uncertainty, and effort (e.g., Madden & Bickel, 2010). Basic operant research was one of the most important early influences on discounting research, which subsequently proliferated to encompass issues of social

importance as diverse as academic procrastination, patterns of electronic commerce, public policy decisions, and several types of psychopathology, including drug abuse and excessive gambling (Madden & Bickel, 2010). An electronic search performed via PsycINFO for the purposes of this essay (using the search terms of *delay*, *temporal*, *probability*, *effort*, and *hyperbolic discounting*) revealed 522 published sources in numerous journals in several disciplines. Discounting clearly has gone mainstream. Yet only 18 (~3%) of the sources just mentioned appeared in EAB's flagship journal, *JEAB*. What are the odds of these few studies convincing an intelligent observer (perhaps an administrator at the National Institutes of Health) that discounting is the province of EAB rather than, say, economists or cognitive psychologists, who also have embraced this topic? To appreciate EAB's historical contributions, one must be familiar with basic animal experiments on self-control (e.g., Rachlin & Green, 1972), and it is not clear how widely read those may be or how well prepared a nonexpert may be to appreciate the conceptual relevance of these studies to human discounting. Even in the unlikely event that historical contributions are properly acknowledged in this case, is that likely to prompt investment in the next pure basic EAB breakthrough that has not yet been extended into the translational realm?

Earlier, the reader may have wondered why I chose to emphasize Skinner's pure basic research over his considerable efforts, in works other than *The Behavior of Organisms* (1938) and *Schedules of Reinforcement* (1957), to speculate about the relevance of laboratory-derived principles to everyday concerns. I did so because of Skinner's tendency to address translational implications in communications other than those that described his laboratory work.

At different times Skinner played the role of both the pure basic scientist and the "someone else" who explores the social relevance of basic science. He thus established a model for dealing with pure basic science and its societal implications in separate steps (and in his translational writings he did not always specify the exact basic science findings that inspired him). This approach can leave the connection between basic science and human affairs unclear to casual observers.² One goal of translational research is to integrate more seamlessly basic science findings with their possible implications.

As Stokes (1997) has noted, one conclusion that governments have drawn, justifiably or not, from the heady period of basic science funding policy following World War II is that the practical return on investment in pure basic research is unpredictable at best. By the time of Hake's (1982) seminal observations about translational behavior science, society had already begun to lose enthusiasm for the pure basic science model, a trend that continues to the present. In the United States, funding agencies increasingly have sought to support basic research programs in which the connection to practical problems is transparent (e.g., Mace & Critchfield, 2010; Perone, 2002). Investigators who pursue pure basic research are unlikely to fare well in this funding climate. Opportunities to pursue basic behavior research also are being constrained by the closing of animal laboratories (often as an institutional cost-cutting measure) and by a shortage of good academic positions (Mace & Critchfield, 2010). These are precisely the kinds of outcomes that would be expected if pure basic behavior science is seen as an enter-

² Indeed, Baron, Perone, and Galizio (1991) suggested that narrative interpretation is less likely to persuade a skeptic than to promote a believer's "sense of self-satisfaction with the apparent scope of the explanatory principle" (p. 102).

prise that delivers too few benefits to justify support.

Gaining Much, Giving up Little

A translational agenda, like that suggested by Poling (2010), can improve the benefits term in a benefits-to-costs appraisal of EAB research. The link between translational basic research and practical problems can routinely be made obvious, and thus society should have fewer qualms about supporting it than pure basic research of ill-defined practical relevance. Imagine the potential benefits to EAB, for instance, if *JEAB* had become the central outlet for translational research involving discounting.³

A matter of potentially great concern to the basic scientist is whether practical relevance can be embraced without compromising the *basic* character of basic science. I believe that this is a straw-man issue for the simple reason that, unlike with research positions and dollars, there is an unlimited supply of research questions that concern fundamental mechanisms of behavior. There will never be enough time, resources, or investigators to tackle all of them, so some discretion is needed in deciding which questions to pursue. A translational perspective merely tweaks this reality by suggesting that, among research questions that concern fundamental mechanisms of behavior, we favor those of obvious relevance to practical problems, and we speak overtly of their relevance. It is not

necessary, therefore, for basic scientists to abandon their quest for abstract principles to become obviously “relevant to significant actions of [individuals] in their natural environments” (Poling, 2010, p. 9). Rather, it is possible to pursue multipurpose knowledge that applies transparently to both theory and practical problems.

Stokes (1997) used the term *use-inspired basic research* for the approach I have just described, and suggested as a paradigm example the work of microbiologist Louis Pasteur, who devised important laboratory studies to address questions that were raised in industrial beet sugar fermentation. Pasteur’s investigations employed rigorous methods compatible with laboratory best practices of his time, and he conceptualized his questions in terms of basic biological mechanisms of pure basic interest. More recently, virology research, launched in the wake of the HIV/AIDS epidemic, has employed basic science strategies to answer questions of critical practical import (for a brief summary, see Mace & Critchfield, 2010). In contemporary terms, this work, like Pasteur’s, was translational in inspiration but largely indistinguishable from other basic research in terms of method and focus on fundamental principles. This suggests one useful formula for devising translational research programs in the EAB laboratory, and fortunately, this is not entirely uncharted territory. Throughout EAB’s history, use-inspired research has emerged occasionally from basic laboratories (for recent examples, see Erjavec, Lovett, & Horne, 2009; Fantino & Kennelly, 2009; Guinther & Dougher, 2010; Habib & Dixon, 2010; Mace et al., 2010; Stewart, Barnes-Holmes, Roche, & Smeets, 2002). The present proposal, therefore, is relatively modest: More such work may be needed to demonstrate why society should invest in EAB.

³I have mentioned that Skinner set a precedent for distributing his basic science contributions and his translational insights through different types of publications. In large measure Skinner’s approach foreshadows the discounting literature, to which a number of investigators trained in EAB have contributed translational studies, though rarely in *JEAB*. A similar state of affairs exists in behavioral pharmacology, another mainstream translational topic that owes a great historical debt to EAB (e.g., Laties, 2003) but appears only sporadically in *JEAB* today.

WHY CHANGE IS HARD

Pasteur's example illustrates how a translational agenda can both address social relevance and avoid the overshadowing of basic science considerations by applied concerns. It does not, however, indicate how individuals like Pasteur arise in the first place or how they accomplish important translational work, matters of some consequence if the rate of use-inspired basic research is to increase. Below I address some potentially relevant issues.

Translational Expertise

In recent years, relatively few individuals have been responsible for producing much of the translational research published in behavior analysis (e.g., Critchfield & Reed, 2004). This suggests that translational expertise is not an automatic consequence of the way in which behavior analysts typically are educated, in terms of both their formal academic training and the literatures with which they become conversant afterward. For EAB researchers, thinking translationally means mastering two worlds, that of the basic laboratory and that of at least one important applied problem. For example, a behavioral pharmacologist needs to master the basic science fundamentals of both behavior and pharmacology, along with, in many cases, a disorder for which drugs of interest may be prescribed or to which excessive drug taking is integral. To be clear, basic researchers need not function in the applied sector (e.g., provide services), but they must understand a given practical problem well enough to decide what aspects of basic research and fundamental principles probably are relevant to it.

Casual observation suggests that many scientists are trained primarily as members of either basic or applied communities, and afterward tend to read within their existing areas of

expertise (e.g., Hake, 1982). This does not reflect laziness or lack of curiosity but rather the pragmatic challenge of allocating limited time to a burgeoning scientific literature. Who among us can fully master one scholarly area, let alone two or more? This constraint exists for basic scientists no matter how enthusiastically they are exhorted to read and think more broadly. Nevertheless, basic scientists who do not read applied research or interact in some way with practical problems forfeit opportunities to learn about the issues that most perplex society, which may help in developing and understanding the practical implications of their own work (Mace, 1994).

Translational research also must be *explained* in ways that are consistent with the language, values, and cultural practices of a practical society (e.g., Rogers, 2003). To illustrate, Table 1 provides examples of introductory comments from translational articles in *JEAB* that addressed fundamental principles yet left no mystery about the everyday phenomena to which the principles of interest were thought to apply. Basic scientists who do not interact with applied literatures and verbal communities may lack the skills necessary to establish social relevance in this way.

Outside the behavioral sciences, the problem of limited individual expertise often is solved through collaboration of individuals with different types of skills (e.g., Mace & Critchfield, 2010). In areas like defense industry research and development, it is widely recognized that few people are jacks-of-all-trades, so innovation often is engineered through teams (Critchfield & Reed, 2004). A fascinating side benefit of this approach results from the fact that a collaborative team may exist only for the duration of a given project, after which it disbands and its members, now better informed about the expertise held by other

TABLE 1

Examples of Establishing a Practical Context in Which to Evaluate Basic Research Findings

<i>JEAB</i> article	Excerpt from Introduction section
Guinther and Dougher (2010)	Both eyewitness testimony ... and self-reports of childhood sexual abuse ... have proven to be surprisingly fallible. Research in both of these domains has identified circumstances under which the accuracy of reporting is far below what would be expected based on everyday experience, suggesting that great care need be taken by police, therapists, or anyone who wishes to obtain accurate information without distorting the recollection of subjective experiences. (p. 329)
Kohlenberg, Hayes, and Hayes (1991)	In natural language, however, contextual control over word meaning is typically supplied by other words. For example, compare the different meanings of the word bat in the following sentences: "Babe Ruth held the bat firmly" and "Dracula held the bat firmly." In this example, bat is in an equivalence relation with a piece of wood or a flying mammal, depending upon other words (Babe Ruth or Dracula, respectively) as contextual stimuli. (p. 505)
Dymond, Bateman, and Dixon (2010)	Increases in the prevalence of adolescent gambling are of concern because those who begin gambling at a young age are more likely not only to develop later pathological gambling ... but are also at greater risk for behavior disorders, including conduct disorder and substance abuse. (p. 353)
Derenne (2010)	Peak shift has ... been invoked to explain the human tendency to find ... exaggerated features ... to be more aesthetically pleasing than natural representations of appearance. ... Similarly, some textbooks on learning theory have suggested that a man who had a positive relationship with a woman with dark brown hair (S+) and a negative relationship with a woman with light brown hair (S-) should prefer women with very dark hair ... or that a man who had a positive relationship with an extrovert and a negative relationship with an introvert should prefer women who are very extroverted. (p. 486)

team members, connect with other collaborative teams focusing on different projects. This is thought to engender valuable cross-pollination of ideas across areas of specialization (Shapero, 1966).

Collaborating with applied behavior analysts is a good way for EAB investigators to enhance their translational thinking and impact (Mace & Critchfield, 2010), but this may happen only to a limited extent in contemporary behavior analysis. Inspection of the flagship behavior analysis journals will reveal that neither basic nor applied articles tend to include coauthors of the other persuasion (although in recent years applied behavior analysts have done a better job of recruiting basic input than the other way around; see Mace & Critchfield, 2010).

Habits are formed early in careers, and consequently a logical place to begin promoting "translational behavior" is during graduate training (Critchfield & Reed, 2004). Here it may not be enough to require basic *and* applied experiences. Also valuable may be experiences in which basic and applied perspectives are explicitly melded. I fondly remember a course (taught by Bill Redmon) during my own training at West Virginia University in which, for each of several topical areas, basic and applied articles were read and discussed together, to fascinating effect. Nominally, the course served to expose my cohort of basic research students to applied work, but in hindsight I believe its most important function was to force us how to talk to, and understand the concerns of,

the applied students who also took the class.

Experimental Methods

For the sake of narrative simplicity, let us assume that through careful teaching and mentoring we can create a sizable cohort of future EAB investigators who embrace a translational perspective. Those investigators will have at their disposal a powerful arsenal of investigatory tools that have been perfected across the better part of a century (e.g., Iversen & Lattal, 1991; Sidman, 1960). It should be noted, however, that the preferred methods of EAB have been shaped mainly by the demands of pure basic research. Whether the same methods prove to be suitable for all instances of use-inspired basic research remains to be seen. Below I discuss just a few potentially relevant examples.

The inductive strategy. Arguably, the dominant general strategy of research in EAB has been Skinner's inductive approach ("When you run into something interesting, drop everything else and study it," 1956, p. 223). Skinner extolled the inherent joy of discovering order in behavior and famously argued for structuring research programs to maximize the contingency shaping of investigator behavior by the behavior under observation. Within this approach to research, one research question is as good as any other as long as orderly functional relations are revealed. As noted above, however, within a translational agenda, the selection of research questions that bear on fundamental phenomena is not arbitrary; those of practical import are preferred. This selection bias applies equally to the initial choice of research questions and to decisions about how to proceed when "something interesting" arises.

Nonhuman subjects. EAB was born in the animal laboratory (Skinner,

1938), and nonhumans confer many advantages to basic research. For example, behavior science is most effective when behavioral variability under control of experimental factors exceeds that under the control of extraexperimental factors (e.g., Sidman, 1960). Nonhuman subjects, with their limited and well-controlled preexperimental histories, yield a favorable denominator in the experimental-preexperimental ratio compared to adult humans, who enter a psychology experiment with 100,000 hr or more of unspecified learning history (e.g., Branch, 1991). Nonhuman subjects also show up reliably and well rested for research sessions and are not distracted from experimental tasks by a recent fight with a girlfriend, indigestion from a pizza and pork rind breakfast, or an MP3 player that was smuggled into the laboratory. It is easy to see why they have been preferred.

Whatever their advantages for pure basic investigations, nonhuman subjects create potential disadvantages for translational research. As Hake (1982) noted, for example, verbal and social behaviors that are seen most often in *Homo sapiens* provide a potentially fascinating focus for basic research. Such behaviors also are central to many topics of practical importance. The challenges of studying such behaviors in other species will not be addressed here except to say that research always is difficult; making it more so by using subjects that are ill suited to the research question is not advised.

Keep in mind that a translational agenda includes not only addressing practical problems but also convincing society that basic researchers do something worth supporting. Even when "species typical" behaviors in *Homo sapiens* are not the focus of study, there may be advantages to studying humans. Although operant principles often appear to be shared across species, interspecies generality should be demonstrated rather than

assumed. Some benchmark effects in nonhuman behavior, like scalloping on fixed-interval reinforcement schedules,⁴ have been difficult to replicate in humans (e.g., Weiner, 1964), and some of the more exciting recent discoveries in EAB, such as momentary patterns of postreinforcement preference shift under concurrent contingencies (e.g., Davison & Baum, 2000), have not been extended to the human laboratory. At the least, EAB's reliance on nonhuman subjects provides encouragement to skeptics who are inclined to believe that because "fundamental laws of reinforcement were derived [from] the behavior of captive starved lower animals," they are "generalizable to the conditioning of single captive starved lower animals and mentally retarded and very young human students, but not beyond" (Friedman & Fisher, 1998, pp. 233–234). Casual experience suggests that these skeptics are depressingly numerous.⁵

Steady-state methods. Nonhuman subjects have played a critical role in the development of EAB's primary approach to dealing with behavioral history that cannot be controlled. Steady-state methods often allow nuisance effects of preexperimental history and carryover effects from prior experimental conditions to dissipate or be overwhelmed by exposure to current experimental contin-

gencies.⁶ Steady-state designs may accomplish this *if* experimental conditions can continue for as long as necessary to achieve equilibrium in the behavior under study (Sidman, 1960). The resulting experiments, of course, can be quite extended, in some published instances approaching 100 hr of session time per experimental condition. When human subjects are of interest, however, it may be necessary to find ways to detect effects of interest more quickly. Except under extremely unusual circumstances (e.g., Findley, 1966), humans are unlikely to consent to be studied for such extended periods. EAB's strong reliance on steady-state methods may help to explain why human research has always constituted a minority approach in the basic science wing of behavior analysis (e.g., Dymond & Critchfield, 2001).

In noting this translational drawback of extended steady-state methods, I do not advocate for poorly controlled research in which conditions are truncated out of convenience rather than scientific purpose. I share with Sidman (1960) and many others a concern for making sure that valid conclusions are drawn from each experimental condition. Yet it is possible that some animal experiments have become quite lengthy, not because they have to be, but simply because they can be. By spending

⁴ Verbal mediation often is suggested as a reason why humans fail to show the same kind of temporal patterning as nonhumans. An interesting point of debate is whether verbal behavior is an artifact of uncontrolled human learning histories or part of a fundamental interaction between different types of behavioral repertoires. In the former case nonhumans might be preferred for basic research, but in the latter case humans would almost certainly be required.

⁵ For example, compare the Friedman and Fisher (1998) quote with Chomsky's (1959) dismissal of Skinner's (1957) *Verbal Behavior* on the grounds that "the insights that have been achieved in the laboratories of the reinforcement theorist can be applied to complex human behavior in only the most gross and superficial way" (p. 28).

⁶ Part of the value of steady-state designs to pure basic research is that, in the absence of other influences, even weak (but theoretically important) controlling variables can yield obvious effects. Like most readers I believe that behavior principles are highly general, and I expect most effects revealed in the laboratory to occur outside the laboratory. At issue from a translational perspective is whether the effects seen in some laboratory experiments are robust enough to make a difference in everyday circumstances, that is, to supersede the influence of many other variables. Following my contention that basic research should pursue multipurpose knowledge, ideally basic research will address phenomena that are simultaneously important to theory and potent enough to bear on practical problems.

relatively little time with human subjects, EAB researchers may have avoided the need to figure out how to obtain valid observations in a relatively short time. And, by the way, the benefits of doing so may be generic (i.e., not specific to human research). Given that research resources are scarce, the faster one can conduct a good experiment, the more experiments that one can conduct and the larger the corpus of research that can be produced by a fairly small community of investigators.

Once EAB investigators figure out how to do faster experiments, their work also will become more appealing to researchers in several disciplines in which practical constraints demand brief observations. For example, because sophisticated brain imaging is quite expensive, many neuroimaging studies employ behavioral tasks that are quite brief, sometimes lasting only a few minutes. Even the most efficient steady-state designs may be poorly matched to this kind of research, and yet, when a behavioral approach has been wed to such contemporary methods the results often have been valuable (e.g., Habib & Dixon, 2010; Schlund & Cataldo, 2005). Thus, determining how to do brief-but-good basic research may create the side benefit of promoting more interdisciplinary collaboration.

A final concern is that steady-state methods may discourage attention to effects that are fleeting but important to both theory and the everyday world. It might be useful, for instance, to understand the variables that control choice making when a particular decision can be made only once and for which relevant behavioral history is incomplete. Such conditions may apply to many actions of considerable societal interest, and the relevant research has received considerable attention from both scholarly and lay audiences (e.g., Ariely, 2008; Lehrer, 2009; Thaler & Sunstein, 2008; Tversky & Kahneman, 2000). This research tends not

to be found in the basic behavior analysis literature, which focuses instead on choices that are made repeatedly and with the benefit of extensive relevant learning histories (e.g., Mazur, 1991).

Single-subject experimental designs.

In behavioral research, steady-state methods are closely associated with single-subject experimental designs in which critical effects are replicated in just a few individuals (Sidman, 1960). Recalling the point that science occurs in social context, it is important to acknowledge that, for better or worse, group-comparison designs that involve large numbers of subjects have become the norm in psychology and many related disciplines. Within this mainstream, single-subject research often is misunderstood and, according to several methods textbooks I have seen, may be viewed as similar to a minimally controlled case study. The goal of achieving influence (which includes convincing others that what we do is both important and believable), therefore, may sometimes be advanced by using designs that others (e.g., policy makers and other consumers of science) find convincing.⁷

Another use for group-comparison designs is to study behavior that is not understood well enough to control with great reliability in the laboratory. Such behaviors may be of special interest in translational laboratory studies. One possible example is the self-editing of verbal behavior, a phenomenon that many (including Skinner, 1957) have suggested is fundamental to understanding human communication. Cogni-

⁷One of the major arguments against group-comparison designs is that they can lead to different conclusions than those that are reached based on close inspection of individual data. A few relevant examples can be cited, but it is unclear whether they define the rule or represent exceptions to it. In any case, there is nothing to prevent concerned investigators from examining the individual data on which group analyses are based.

tive psycholinguists have devised several means of experimentally inducing self-editing in the laboratory, although none of these produce self-editing in all participants, and the effects are not necessarily replicable within subjects (see Epting & Critchfield, 2006). The norm in psycholinguistic experiments, therefore, is to manipulate experimental variables across groups and compare in terms of the prevalence of self-editing. Because single-subject designs depend on repeated measurement of the same behavior in individuals (Sidman, 1960), it is unclear whether any existing procedure would permit self-editing to be studied in single-subject designs. Given this status quo, behavior analysts might simply refuse to investigate self-editing on the grounds that it is an unreliable phenomenon, functionally ceding this interesting topic to other scholars. Alternatively, behavior analysts might resolve to devise better methods before proceeding, although this is a painstaking endeavor with uncertain endpoints (see Hyten & Chase, 1991). A final option is to proceed with studies that use, and perhaps simultaneously attempt to improve, existing procedures, at least temporarily embracing the group-comparison approach. This would represent a departure from normative practices in basic behavior-analytic research, but with the benefit of helping to establish the relevance of basic behavioral research to a socially important topic.

CONCLUSION

What is wrong with the experimental analysis of behavior? In one important respect, nothing whatsoever is wrong. EAB succeeds impressively as a means of posing theoretical questions and answering them under controlled laboratory conditions. In surveying EAB generally, by no means do I suggest that pure basic research is pointless or should cease. This type of research is fun and

engaging (as per Skinner, 1956) and is critically important to theory (which I believe is critically important to society, even if those who control funding do not always see it that way).

Earlier I quoted selectively from a well-known passage in Skinner's (1938) *The Behavior of Organisms*. Here is the full passage:

The reader will have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs. But it is a serious, though common, mistake to allow questions of ultimate application to influence the development of a systematic science at an early stage. (p. 441)

The present essay can be summarized partly by proposing that EAB, now in its eighth decade following the publication of *The Behavior of Organisms*, is no longer at an early stage. Much has been learned since Skinner's first laboratory work that can shed light on human affairs. Applied behavior analysis documents this well, but the closing of an idiosyncratic era in federal science policy that benefited Skinner and many of his intellectual descendants may mean that human affairs have become an important litmus test for basic behavioral science. If Stokes (1997) is right, no longer can basic scientists depend on others to demonstrate how their work provides society with a return on investment, as per the indirect benefits assumption.

To be clear, individual investigators always remain free to pursue the research of their choosing. Those who prefer to conduct pure basic studies and have no worries about securing employment or research resources may comfortably ignore the present discussion. Others with pure basic interests, however, may find that their prospects for support improve when studies are tied clearly to practical themes of broad interest. Overall, EAB's track record is incon-

sistent when it comes to demonstrating the social relevance that contemporary society appears to demand of basic science. Translational (use-inspired) basic research may not trigger a stampede to the doors of EAB investigators, but this kind of research at least addresses the core problem of exploring social relevance, potentially without compromising an investigator's capacity to explore theoretical issues.

To reiterate a point, we cannot simply will EAB into greater social relevance. If social relevance is the goal, we should begin by determining whether individual investigators have the skills required to pose, and to answer, questions of transparent social relevance. In the former case, the formative experiences and collaborative habits of basic scientists are worth examining. Translating may require a specific conceptual set that does not automatically emerge from basic science training. In the latter case, a distinct methodological tool kit has allowed EAB to succeed as pure basic science, but with limited translational work originating in the EAB community, it is impossible to judge how universally adaptable this toolkit may be to translational research. Prospects for societal impact are greatest if translational research is not limited to topics that can be addressed easily using currently preferred methods.

These are heady problems, but defining them places us closer to solutions than simply deriding basic scientists for their "esoteric" interests. By understanding the factors that make EAB what it is today, we position ourselves for a stimulating conversation about how EAB can best contribute to a future that includes enhanced social relevance.

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